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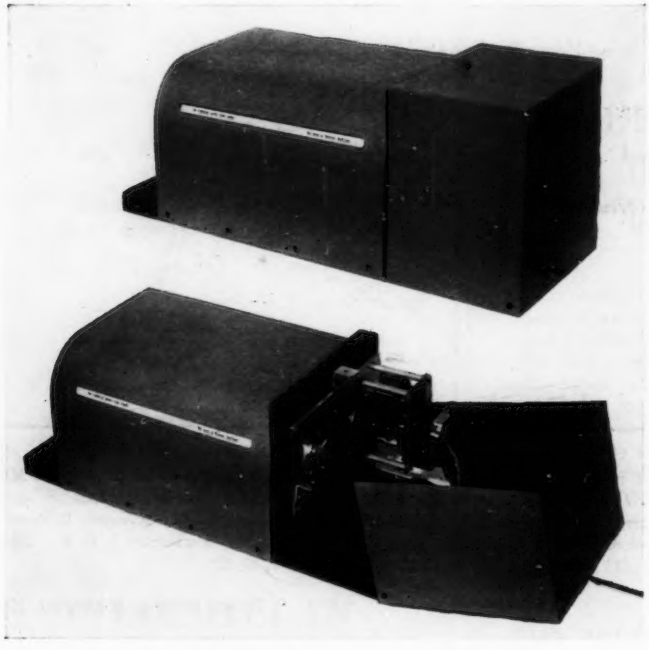

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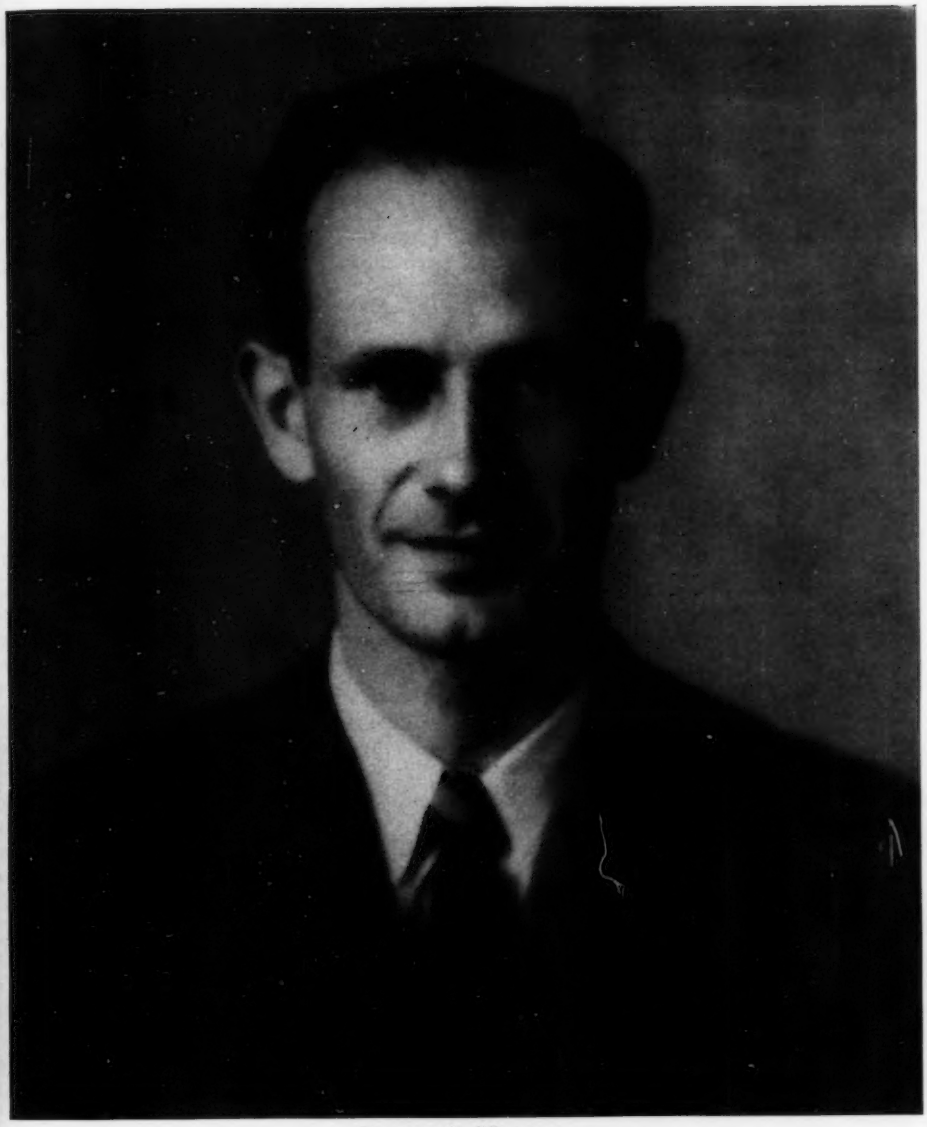

DONALD G. MARQUIS
Professor of Psychology, University of Michigan
President of the American Psychological Association, 1948

RESEARCH PLANNING AT THE FRONTIERS OF SCIENCE[1]

DONALD G. MARQUIS

University of Michigan

PSYCHOLOGY today faces a task of a different order from any before. During its brief period of development it has been relatively free to grow at its own pace and in self-chosen directions. Nurtured by steadily expanding college enrolments and with occasional odd-jobs in industry and the clinic, psychology developed its methods, facts, principles and technical skills to a point that enabled it during the crisis of the last war to join with the older sciences in making a significant and effective contribution to the common goal. Psychology has demonstrated its maturity; it can now expect to be called upon to assume the responsibilities of an adult science.

The nature of these responsibilities is now being made clear. Many of the crucial problems facing our society are problems of human relations and social organization. The great advances which have been made in industrial production, in transportation and in communication do not by themselves guarantee the general welfare—indeed, they create such rapid changes in our way of life that the traditional trial and error methods of social adaptation are totally inadequate. Critical gaps in our knowledge are found in mental hygiene, in designing machines with respect to their operators, in industrial relations, in minority tensions, in educational methods, in social and political organization, and in military defense. Leaders in various activities, impressed by the achievements of science in physical and biological fields, are asking what science can contribute in the psychological and social fields.

The call for new knowledge of human relations is not just a rhetorical call—it is backed up by money and other support. The great philanthropic foundations such as Carnegie and Rockefeller have deliberately allocated a larger share of their resources to research in the social sciences. And the national government has called for tremendous expansion in this field. Every one of the federal departments with the exception of the Post Office is now supporting research on human behavior. The Veterans Administration and the U. S. Public Health Service are spending between two and three million dollars annually in developing the field of clinical psychology through fellowships, hospital and clinic positions, research funds, and training grants to universities. The Departments of Army, Navy and Air Forces, now the biggest customers for research of all kinds, have budgeted between 6 and 7 millions this year for studies in psychology and social science which are being carried out partly by military research agencies and partly by contract with university departments and research institutes. There is good reason to believe that the total level of support will have to be doubled or trebled before the clearly recognized needs of the nation will be even approximately met.

This general picture might be considered a source of gratification except for one reservation: there is no guarantee that we can deliver what is expected. Our resources of research know-how are concentrated in the older and better established areas of science while the call now is for knowledge in the less explored fields of human relations and social behavior.

In the title of this paper I used the phrase "frontiers of science." Although all research is by definition at the frontier of knowledge, it seems to me that science itself has a frontier. The scientific method first became clearly established in astronomy, physics and mathematics, and later was extended into biology, agriculture, medicine and psychology—encroaching on areas formerly considered the province of philosophy, religion or practical common sense. In the span of our own lives we have seen this frontier move through the field of psychology. Many of us learned the method of science in studying biology, physics or physiology and brought it to bear on problems of psychology. As late as the decade of the 1920's the most methodologically defensible research was done in the related areas of comparative, sensory or physiological psychology. The frontier has now passed through the areas of learning and of mental abilities and may be recognized somewhere in the fields of interpersonal

[1] Address of the president of the American Psychological Association at Boston, September 9, 1948.

relations and social psychology, with extensions into anthropology, sociology, economics and political science.

Let us recognize clearly that frontier research is not easy. A useful contribution in the field of human relations will loom large in relation to the present meager level of verified knowledge, but it is not a simple thing to achieve. The eager researcher finds no adequate structure of theory to guide his inquiry; not even a terminology with commonly accepted meanings. He is thwarted by the lack of standard measurement techniques for the relevant variables, and dismayed at the absence of even the simplest kinds of taxonomic data on the materials of his study. The scientist who would forsake the security of a well established research field with the prospect of one sound publication a year must have a certain amount of courage or else a full professorship.

It is not surprising to find that the quality and the product of research in frontier fields appears relatively inferior. Although hundreds of competent individuals are publishing thousands of books and articles each year, one cannot escape the impression that it does not always add up to any very clear and usable result. The intriguing problems are so numerous and diverse that research effort becomes scattered rather than focused. And the individual researches are difficult to put into a cumulative integrated structure because the concepts used are not the same, the observational data are not commensurate, and the samples studied are not chosen or described in such a way as to permit combination.

I can find no reason to doubt that steady progress will continue to be made in the research fields which are now being initially explored. Eventually certain concepts will come into common use, definitions and measures will become more standardized, and a coherent body of knowledge will be built. For those who are content to wait patiently for the inevitable blossoming of the science of human relations I have nothing more to say. For those who share my feeling that the urgencies of our contemporary society demand an accelerated rate of scientific advance I propose to discuss some possible steps toward its achievement.

Scientific progress can not of course be speeded up by command nor by exhortation, nor even by money alone. If the number of scientists and the amount of financial support were doubled there would be twice as much of what we have now, but that may not be enough to keep pace with the needs of our swiftly changing society. And since there are very definite limits on the rate of training additional research experts, it becomes imperative to examine any other possible means of increasing the effectiveness of scientific work. It is my thesis that more attention to planning can greatly accelerate the rate of development of frontier research.

Research planning can be carried out at different levels. I would like to distinguish three levels which I will call experimental design, program design and policy design. Experimental design is the planning of a single specific project. Program design is the planning of an integrated set of projects focused on a central problem. Policy design is a new word for overall planning of the distribution of effort among programs, areas or fields. Planning at the first or project level is a universally accepted requirement of scientific method—it is what distinguishes an experiment from objective openminded common sense observation. In the past decade, following R. A. Fisher's monograph (3), we have all become very much aware of the design of experiments, and statistical method is now taught with as much concern for the planning of research as for the analysis of collected data. Psychologists have shown an unusual interest in and capacity for experimental design. Among the younger members of the profession at least we find widespread sophistication on tests of significance, methods of drawing a sample, null hypothesis and factorial design. You can seldom catch a psychologist without an estimate of the error variance.

At the other end of the scale is the level of policy planning of the broad directions of scientific effort. Psychology and the other sciences have had little experience with this type of planning. General trends in the growth of a science have been the complex resultant of a multitude of individual decisions by scientists about their own work, influenced unsystematically by social needs and pressures such as university policy and available financial support.

There is clear evidence now, that with the growing recognition of the relevance of scientific research to national welfare, the major agencies of research support are assuming responsibility for policy planning. Foundations have always distributed their funds in a pattern designed to stimulate work in certain fields. We have seen the valuable results in psychology of the support of child welfare research institutes during the 1920's. Industrial associations,

pharmaceutical firms and organizations such as the cancer or infantile paralysis funds also operate to expand research in selected areas, but so far have had only incidental influence in psychology. The most important planning agency now is the federal government. As it assumes a larger and larger share of the support of scientific research, it establishes planning boards to make sure that funds are allocated wisely. The Office of Scientific Research and Development, created during the last war, undertook responsibility for allocation of money and personnel to the several objectives of the national effort. The OSRD is now succeeded by the Research and Development Board, under the chairmanship of Vannevar Bush, with the task of planning an overall research program for the Army, Navy and Air Force. One of the 15 committees of the Board is the Committee on Human Resources, covering the field of psychology and the social sciences. During the past year the Committee, through its central staff and its numerous advisory panels, has surveyed the current national research activities and has recommended an expansion and redistribution of research effort designed to contribute maximally to both the immediate and the long-range needs of the country. Other evidences of national planning are found in the Bush report (2), and in the report last year by the President's Scientific Research Board (7), and in the recent appointment by the President of an Interdepartmental Committee for Scientific Research. If and when Congress eventually approves the creation of a National Science Foundation there will be an agency with authority and responsibility for planning on an even broader scale.

I do not propose to discuss the procedures or the desirability of policy planning. With the growing tendency toward centralization of research support in federal funds, it is inevitable that allocation of such funds must be made on some basis. At the present time there are no adequate objective procedures for evaluating programs or fields of research, and the most we can expect is that scientists will participate in policy decisions on the distribution of funds. I hope that a few years hence someone will address you on the subject of the criteria and methods of research policy design—but it cannot be done today.

Between the level of experimental design and policy design there is a large gap which neither of them can fill. Policy planning, by virtue of its breadth, can not be a specific guide to actual research. Experimental design, on the other hand, is concerned with the investigation of a restricted question and cannot be a guide in choosing the most significant problems for research attack. I believe that the greatest present need for planning exists at this intermediate level which I call *program design*, and I believe that by exploiting this type of planning we can accelerate the development of frontier research in areas critical for human welfare.

By program design I mean the planning of an integrated series of research activities, focused on a central problem, and involving a number of scientists for several years. This is not, of course, an original idea. There have been outstanding examples of good program design in frontier research, but they are few. Usually they are found in the work of an investigator who keeps his eyes steadily on a central problem throughout his lifetime. One thinks immediately of Ebbinghaus's studies of memorizing, of Pavlov's work on conditioning, of Terman's research on intelligence, of Thurstone's analysis of human abilities, of Hull's and Tolman's programs in rote learning, of Maier's work on reasoning, of Kurt Lewin's studies in group organization. The fruitfulness of such integrated programs of research is what convinces me that it would be worth while to study the characteristics of program design so that the procedure could be more widely used.

I do not believe that it takes a genius to design a research program, nor is it necessary to wait for that rare flash of scientific insight. An example from another field of science may indicate more clearly what I mean. In meteorology there is no theory comparable to that in physics or chemistry. It is conceivable that hundreds of students of weather could carry out in their individual laboratories and field stations a multitude of little unrelated studies. One of them would clock the sunrise in Central standard time; another would report the movement of the moon in Eastern daylight saving time; one of them would record wind velocity on a 7-point descriptive scale; another would devise a windmill gadget and report velocities in reliable but arbitrary units of revolutions per minute. One would study temperature in Albuquerque, another rainfall in Cheyenne. In spite of all the diligence and individual ingenuity of this earnest band of researchers it would be a slow and wasteful process to accumulate useful meteorological knowledge. But because the problem is sufficiently important, the meteorologists have gotten together and agreed to collect

commensurate data by standardized methods in all parts of the world. With these data available there has been steady growth in theory and in the accuracy of prediction. This is an extreme example. In most frontier fields it may not be possible to undertake so comprehensive a program but rather to have many programs proceeding simultaneously, each large enough to provide an adequate test of the conceptual theory involved but permitting several alternative approaches to the same general problem.

What, then, is the essential feature of program design? It is the attempt to plan a comprehensive, integrated series of studies in relation to a particular set of concepts focused on a central problem. It is the attempt to broaden and lengthen the scope of a research sufficiently so that we can tell whether it is really getting anywhere. It is scientific method in its full and complete form. I shall list six steps in program design. The temporal sequence of the steps is not fixed although there is a natural order.

1. The first step may be called *problem formulation.* In the selection of a problem there appears to be no restriction on the kind of topic which can be attacked scientifically. "The religious attitudes of middle-west farmers" is as acceptable a problem as the limits of their visual sensitivity. There are, however, two important limitations governing the formulation of a problem for scientific study. The first is that a value proposition cannot be verified by any present scientific methods. It is necessary therefore to separate the value assumptions from the factual propositions which can be investigated. Most of the problems arising from the practical situations of living come to us with a mixture of value and factual questions. We cannot, for example, test the proposition that spaced practice is better than massed practice. Analyzing this problem into its components, we formulate the proposition that spaced practice results in more rapid learning than massed practice, and we do not examine the question of whether rapid learning is desirable or not. Such analysis of a problem poses few difficulties in physical and biological science but is a frequent source of confusion in dealing with topics like labor relations, race prejudice, and therapy.

A second limitation on the scientific method of inquiry is that the phenomenon under study must exist in replication. There is no known method by which a single unique event can be the subject of research. If we are interested in what appears to be a unique situation, such as the explanation of the suicide of a particular individual or the form of organization of the world state, it is necessary to reformulate the problem in terms of those aspects of the situation which can be identified in several instances. This is a serious limitation on the scope of scientific investigation and one which is not completely accepted by all persons who become interested in the significant and real problems of practical life.

2. The second step is *review of knowledge*—of what has been learned or said by others about the topic. The library is the usual source of this material but personal informants are often utilized, for example, in anthropological field work. Although this step is clearly recognized by everyone, the difficulties of achieving it are becoming serious as a result of the tremendous growth in the literature of every field. Abstract journals, bibliographies, summaries and reviews are important aids in this respect and should be extended into frontier fields more adequately than at present. If research is organized into programs it becomes feasible for each to undertake a comprehensive review of current knowledge, and the Office of Naval Research has recognized this need in arranging contracts which include specific provision for literature summaries.

3. The third step is *preliminary observation* of the events under study. This may be quite unstructured observation which enriches the second-hand library knowledge and suggests new leads for investigation. It may also be more controlled observation in the nature of trying out methods of measurement, formulating codes for classification of data, identifying sub-samples of the population and pretesting promising hypotheses. In its most developed form it may be methodological research—a comparison of two or more ways of describing, measuring or scoring the phenomena. A variety and diversity of observational methods is desirable at this point in order to explore most fruitfully the best possible ways of formulating and categorizing the data.

4. The fourth step is *theory construction*—the formulation of specific and rigorous hypotheses for subsequent test. Now the investigator draws upon everything that he has learned from his review of previous work and from preliminary observation. While theory construction can sometimes be achieved in the comfort of the armchair, I do not want to

imply that it is easy or routine. It is indeed the crucial test of the creative scientist.

I think we are beginning to realize more clearly that a theory, to be useful, must be built around concepts which have specific and unequivocal relation to empirical observation and measurement. Only in this way can theory be verified. Most of what we have called theory in the new fields of psychology has not been of this type. Our theories have a long life span of controversy because of the difficulties of testing them. Their concepts are not such as can be identified or measured. They were invented on the basis of a priori considerations rather than being abstracted from empirical study and as a result, they do not lend themselves to unequivocal test. It is certainly typical, and perhaps inevitable, that the first theories in a new field, since they aim at broad understanding, are speculative and untestable. Man's desire for explanation does not permit a vacuum in his comprehension of the world. Hence the crystal spheres theory in astronomy; the earth, air, fire and water theory in physical science; the demonological theory of mental disease; the social contract theory of the state; and the instinct theory of human behavior. In the history of science it has usually been necessary to abandon such theories completely rather than to try to modify them to a form which permits testing. We can perhaps accelerate the long-range development of scientific knowledge in frontier fields by emphasis upon the construction of theory out of empirical or operationally-defined concepts. Such theories will necessarily be limited in scope. It is inevitable that the most useful theories will, for some time to come, be small conceptual systems dealing with a restricted range of phenomena.

5. The fifth step is *verification*—the testing of the hypotheses or of deductions from them. If our concepts have been properly chosen and if methods of measurement have been devised, we find adequate guides for verification in our present knowledge of statistical methods. This step in program design is the one which has received the most extensive attention and while it is crucial for science, it is not the primary topic of my discussion. Perhaps the most frequent present limitation in the verification stage is found in the selection of a population for study. Too often in the formulation of a problem we fail to think through the definition of the universe to which our theory is applicable and we are apt to overlook the necessity to select a representative sample of that universe for verification purposes. Considerations of convenience, habit and inadequate funds usually determine that the research will be carried out on any available group of college sophomores, nursery school children, or military personnel. Such studies may be very fruitful and have a definite place in the preliminary observation step. Usually, however, they cannot be made a coordinate part of a total integrated body of knowledge.

The processes of verification may take quite different forms, of which the controlled experiment is only one example. Correlation studies, comparative studies, genetic studies, surveys, intensive case studies, and field studies each have a definite place if they are relevant tests of the theory which has been formulated.

The results of the verification process do not all turn out as the theory predicted. This is not entirely unfortunate, since it provides the clue for reformulation of the theory and hence advance in knowledge. The scientist then proceeds with further tests of his revised theory and in this manner theory construction goes along with experiment and observation in a continuous reciprocal alternation.

6. The sixth and last step in program design is *application* of the verified theory. In pure research, of course, application is not involved since the results are used only to modify the theory. But pure research is rare in frontier fields and application must usually be made directly from the research results rather than indirectly through the theory. Application involves three processes itself: first is a value decision on the desired objective, second is the diagnosis of the specific situation, and third is the selection of the relevant verified knowledge and its application. Consider, for example, the clinical psychologist dealing with a client requesting vocational counseling. A decision must first be made whether a job is to be chosen on the basis of salary, status, interests or some combination of factors. Scientific theory does not determine what job the individual should take; it can only point out the probability of certain consequences of a vocational choice. The decision must be made by the counselor or, better, by the client, in reference to his value system. The diagnosis requires pertinent information about the individual and about the possible jobs. It is then possible to apply knowledge such as "the greater the congruence of a person's abilities with the requirements of an occupation, the greater

are the chances of that individual's success and satisfaction in that occupation."

This analysis makes apparent that those persons who are charged with the application of science must have two qualifications over and above their knowledge of research findings: moral responsibility and diagnostic skill. For this reason a class of professionals is created whose long careful training helps to insure effective application of science. The steps in application are usually not very explicit or clear and merge together in what we often call the art of psychological practice. But it seems clear to me that advance in professional psychology will come only with the development of verified theory and of techniques for diagnosing specific situations to determine when the theory is relevant. A complete program design must therefore take account of these needs and make provision for effective application of its results. It is not enough to establish the fact that training first-line supervisors in human relations will result in improved employee morale. If application is to be successful the research program should include studies of the acceptability to management and to unions of supervisor training, and manuals for the training itself. If the nature of the optimal training is different in different situations, there must be diagnostic measures for identification of those situations.

Here, then, is the pattern of program design—the strategy which supplements the tactics of research planning. Its objective is to promote in the undeveloped frontier fields of research what comes naturally in well-established fields. The distinction I am making between effective research and less effective research is not based on the field or topic or practical significance, but only on the basis of its potential incorporation into the body of scientific knowledge. Some studies become a part of the developing science; others are not only forgotten but they leave no trace of any influence. Science can well afford to support work on wild guesses and bizarre hunches, but it cannot afford the wasted effort of indeterminate research—the kind which is not carried far enough to find out whether it is any good or not. If a particular research project cannot be clearly related to an existing body of rigorous concepts and theory—and this is the defining condition of frontier research—it is then necessary to design and carry out a program of research. Anything less is apt to join the company of incomplete and forgotten research.

The responsibility for program design falls clearly on the original researcher. Often a suggestive study ends with the statement that "present results are inconclusive and it is hoped that future investigation will throw more light on the problem." I have not made a tally, but I suspect that in nine cases out of ten no other researcher will take up the challenge.

It may be worth while to look briefly at some of the characteristics of research which does not fulfill the criteria of complete program design. Such research is often distinguished by exceptionally competent achievement of one of the six steps in program design but is incomplete with respect to one or more of the other steps. There are certain types which occur frequently enough so they are easily recognizable.

One of these, which I might call *wisdom research*, concentrates on steps 2 and 3, with thorough library study combined with first-hand experience and unsystematic observation. This paper is an example of what I mean and many of the essays and books in anthropology, sociology and political science are of this kind. I do not mean to imply that they are not worth while, but only that as science they are incomplete. Such studies are valuable guides to immediate understanding and fruitful sources of hypotheses, but the absence of systematic data precludes any verification in the scientific sense. It would be unfortunate if good work of the wisdom type were permitted to block the best work of the scientific type.

Another frequent kind of incomplete program is one which fails to formulate a central problem. Although there is collection of numerous data, there is no adequate criterion to which the data can be related. We might call this *unfocused research*. For example, one might lay out an elaborate research to compare lecture with discussion methods of teaching, arranging equated groups of students, random assignment of instructors and elaborate records of teaching procedures. But what will be the criterion of better teaching? It will probably turn out that the students will do equally well on the final examination, at which point we protest that that is no measure of what we mean by good teaching. But unless such a measure is included in the research, the extensive data serve no function. Sometimes the failure to identify and measure the ultimate criterion is an oversight from lack of plan-

ning; more often it is just too difficult to measure as in the case of the air crew selection research program (4). But whatever the reason, we will not have adequate program design in problems such as counseling, supervisor training and leader selection until adequate criterion measures are included.

Practical research is a special case—it may be complete with respect to the specific local problem which it undertakes to investigate and at the same time be incomplete from the standpoint of its contribution to the development of science. When existing knowledge does not provide a solution to some immediate and local question it becomes necessary to try out new hypotheses. Although the new knowledge may be adequate for the solution of the specific problem, it is not usually in such form as to permit its general application. For example, the research division of company A may find out that music during the morning rest period in the shipping department results in increased output. This finding is all that is necessary to answer the particular problem, but its application beyond that department is limited. There is no basis for diagnosing other situations to determine which ones would be similar and which different.

The distinction between practical research and theoretical or pure research is a matter of degree—the degree of generality of the results. Theoretical research often finds useful practical application, and there is no necessary reason why practical research should not have theoretical significance. But theory is always general, and practical research can contribute to theory only if deliberate design provides adequate statement of the variables in general terms and verification with samples that can be related to known general universes.

Descriptive research is another example of incomplete program design. A full, accurate, unbiased record of observations is of course desirable, but it does not constitute the ultimate objective of research. Examples of descriptive research are often found in clinical case studies, community studies and public opinion polls. Such data are invaluable when they are collected in order to test theoretical propositions, but by themselves they represent an incomplete scientific program. It may be worth while to note the several places in program design where description is appropriate, using clinical case studies for an example. They may be useful in step 3 as a rich source of theoretical concepts and hypotheses; they may be useful in step 5 for verification, provided that commensurate data are secured from a number of individuals; they may be valuable in step 6 as a guide to the diagnosis which is necessary for application of scientific knowledge. The further possibility that case study can be used to construct and verify a theory for a single individual has been explored by Allport (1). The necessary replication of instances is secured by observing repeated behaviors of the same individual, and this becomes an example of practical or local research.

A special case of descriptive research is that in which the investigators seem rigidly tied to a particular technique or method which is applied over and over again to new populations without sufficient regard to its adequacy for the problem being studied. There was a period which most of us can remember when this seemed to be true of animal maze research and conditioning research. And there may be some who wonder if there is similar danger in the present utilization of the Rorschach test, attitude tests, or sociometric ratings. Another evidence of rigidity in research planning comes from the habitual identification of a method with a particular kind of problem. Because a question falls in the field of clinical psychology does not necessarily mean that methods for its investigation are limited to diagnostic tests and interviews. It is encouraging to see more and more examples of the use of laboratory methods in clinical problems, of clinical methods in social problems, and of social methods in industrial problems.

Theoretical studies of a certain kind represent another type of incomplete research. Theory is an indispensable part of science but the elaborate analysis of concepts which have no clear referents in observation or measurement has limited utility in the development of a scientific body of knowledge. Such an analysis provides a systematization of a field which leads to a feeling of understanding but which carries no very clear guides for the next steps in the scientific process. An outstanding example of the highest type of theoretical analysis is found in Charles Morris' recent book on *Signs, Language and Behavior*. If I am right, his book will stand for many years as the best treatment of the subject, but the developments and changes in our theory of language behavior will find their origins elsewhere.

My last example of incomplete research may be called *critical ratio research*. There are many studies impeccable in their experimental design, unassailable

in their careful controls, ending up with a significant difference at a probability level less than .01—and not much else. I sometimes wonder if the critical ratio has been more harmful than helpful in its seductive implication that it signifies a completed research. If we find that there is a significant difference between carpenters and machinists in their attitude toward unionization, we have established a scientific fact, but what do we know? We know very little about the differences between carpenters and machinists if that is what we are interested in, and we know only a tiny fraction of what determines attitudes toward unionization, if that is what we are interested in. Perhaps it may help us to see our problems in their totality if we make more widespread use of the technique of expressing the effect of a variable in terms of the proportion of the total variance which it determines.

It should be apparent now that the burden of my thesis is that each phase or step of scientific research is essential and valuable, but that only in planned combination do they yield the kind of knowledge which becomes a part of the body of science. The pattern of total research is not easy to achieve and it involves definite compromises and limitations. In order to secure commensurate data on many instances for verification it is necessary to give up the ideal of complete description. To get a theory which is testable it is necessary to sacrifice breadth and scope. To do research which is relevant to the critical problems of practical living it is necessary to give up some degree of rigor and the security of working in the better established fields. To achieve the complete pattern of program design it is necessary to give up the desire to get a scientific answer tomorrow to problems like labor unrest, divorce, or world organization.

I would like to be able to suggest some mechanisms and procedures which would be helpful in program design. Although I have participated in planning several researches, I do not have any systematic data on the best ways of doing it. Certain procedural necessities, however, seem to follow naturally from the conception of program design.

The first requisite of program design is that projects must be planned on a bigger scale than that to which we are accustomed. They must extend over a period of years and must involve a number of investigators if they are to meet the requirements. And if they are going to deal scientifically with critical current problems they must have facilities for the extensive collection of data on adequate samples of the population studied. Under present conditions of scarcity of research talent it is desirable to make maximal utilization of every competent researcher by the provision of sufficient assistants, clerks and technicians. In comparing several projects, I have found that it is possible to plan an effective rate of expenditure of $15,000–$20,000 per year for each experienced professional scientist.

A program of such size does not offer any serious problems of administration but it requires the services of a director who will coordinate the planning and the component projects in such a way that the focus of the research is always clear. The professional personnel may number three, four or five to secure a well coordinated yet diversified group. The diversity of backgrounds is a particular advantage of program research.

I am convinced that planning by a group of scientists is superior to individual planning. It seems desirable to take advantage of a variety of skills and experiences in order not to overlook any possible hypotheses and techniques. In talking with persons who have had experiences in planning, I have not found one who did not believe that the outcome of group discussion was superior to the ideas with which he went into the conference. In the planning phase the research team may be supplemented by consultants who are appointed specifically for this function. Such a procedure enables the research to take advantage of the abilities of university staff members whose teaching duties do not permit more than part time or occasional association with the project. The advantage of a diverse team of scientists will be equally clear in the preliminary observation and verification steps. The available research methods are many and no one person is master of all those which may turn out to be relevant for the research. Frontier research is frequently interdisciplinary. The traditional academic fields of knowledge are a reflection and crystallization of the work of the past fifty years, but frontier research is breaking into new problem areas.

The program design should contain a schedule of the several phases of the work. The time allotted to preliminary library work, observation, planning, methodological work, and pretesting should undoubtedly be greater than is usually allowed. In many instances this phase will be longer than that devoted to collection of verification data, and it seems reasonable to expect a research project of

the size we are talking about to spend at least a year in preliminary work. When the verification stage is once begun the concepts, measures and hypotheses are no longer changeable. A continuous project might very well plan for a recurring cycle of operations in which planning, data collecting, and application are repeated more than once. This enables each phase of the program to profit from what is learned in the other phases. It helps to guard against the occasional tragedy in which, at the end, you discover that you have failed to take account of the most important variable.

Research planning today should also make provision for the systematic training of young scientists, in view of their current shortage. Advanced graduate students on half-time assistantships and post-doctoral psychologists on well-paid internships are the most available possibilities for training. Training in program or large scale group research is not the traditional pattern for the doctoral dissertation, but I hope that graduate schools will recognize the value of such training and permit reasonable flexibility in their requirements. I shall count it a mark of distinct advance when some university awards the doctoral degree to three students who worked together to achieve a significant program of research and submitted only one thesis.

Program design, with the objectives and procedures which I have tried to outline, is not intended of course to replace individual and small scale studies. This would be impossible as well as undesirable. It is intended rather that an increase[d] number of large and well planned research program[s] will carry the scientific development of a frontier topic to the point where theory and data can provide guides for the individual researcher and make possible the incorporation of his work in the growing integrated body of scientific knowledge. The urgent requirement for new verified knowledge of interpersonal relations and social organization calls for acceleration in the development of the frontier fields of psychology, and if we are to meet our responsibilities we must exploit every possible means of speeding this advance.

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AN ANTHROPOLOGIST LOOKS AT PSYCHOLOGY[1]

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AN ANTHROPOLOGIST looks at psychology as a cat may look at a king. You are more numerous and more prosperous than we. The diversity of your specialties is more imposing. The rigor of your best established method far surpasses our rough and ready skills. Some sectors of psychology have developed at least the beginnings of true scientific theory, whereas our most successful efforts have the character of careful description, on the one hand, or of artistic insight on the other. And yet it appears to some of us that anthropology has more deeply influenced psychology than academic psychology has influenced anthropology.

The history of the relationship between the two disciplines contains some curious items. It is perhaps symbolic that Kant's last published work was called *Anthropologie* but was actually a treatise on psychology. Throughout the last hundred and fifty years there has been fairly consistent, though often grudging, recognition that psychology and anthropology were trying to answer some of the same questions by different means and that some of the central problems in each field could never be fully solved without data and concepts from the other. In the days of Wundt and during the period when Boas and Cattell were in constant intellectual association the boundary between the two sciences was somewhat fluid. One of the founders of American anthropology, Clark Wissler, took his Ph.D. in psychology.

In spite of the linkage in the National Academy of Sciences and in the Division of Anthropology and Psychology of the National Research Council there followed almost a generation during which effective collaboration was minimal. There was some interstimulation in the fields of mental testing and so-called "racial psychology," as in the work of Garth and Blackwood. There was the brilliant work of Bartlett and his associates on cultural factors in perception. For the most part, however, the relation was that of respectful, ceremonial avoidance.

To some extent this was due to the fact that each group was busy cultivating its own garden. The anthropologists, in particular, were a small group, and they were obsessed with the urgent necessity for recording as many cultures as possible before these were destroyed. This descriptive task seemed more compelling than broad theoretical issues and interdisciplinary cooperation. But there was more to the story than this. The florescence of brass-instrument psychology and the later fashion of rampant behaviorism repelled most anthropologists. The anthropologist had his experience with human beings in the complex context of actual life—not in the laboratory. He had a not very well-formulated notion that human behavior consists in patterns as well as in events and a consequent distrust of isolating experiments, however rigorous. He felt that no valid theory could be based upon radical behaviorism because he knew that acts have arbitrary, symbolic meanings that cannot be recorded by the most acute ear or eye nor by the finest instruments.

To those few anthropologists who seriously concerned themselves with the matter at all, the academic psychology of roughly the first third of this century seemed to be preoccupied mainly with investigating trivial problems by increasingly refined methods. To the extent that this was true it may have been because psychology was striving fiercely to emancipate itself from what Henry Murray has called "its possessive mother, philosophy, and its dogmatic grandmother, theology." Anthropology stemmed largely from the natural sciences and perhaps for this reason felt less compulsion to prove its respectability. Anthropologists were also rugged individualists or even had a touch of the eccentric about them—or they would not have been attracted to the study of what was unusual, not to say odd, from the standpoint of their own cultures.

It would be presumptuous of me to comment upon the contributions of psychology to general knowledge. I content myself with recording the fact that between Wundt and about 1930 academic psychol-

[1] An address written for presentation at the meeting of the American Psychological Association, Boston, Massachusetts, September 8, 1948.

ogy had singularly little influence upon anthropology. A few anthropologists tried to test Galton's studies of color and number symbolism ethnographically, and Lowie did some field work under the influence of Fechner's ideas on aesthetics. Some psychological data drifted through the wall between the two disciplines, but almost the only postulate or concept that appears to have been seized upon by anthropologists was Shand's notion of the sentiments. This may well have been due to the ignorance or obtuseness of my brethren. At any rate it is also a fact that only with the advent of Gestalt psychology and the delayed diffusion of Freudian psychology was a wide receptivity re-awakened. Gestalt psychology was congenial because it seemed to fit with and provide the possibility of explanation of the anthropological discovery that prediction of cultural dynamics is impossible without a knowledge of the organizing principles of a culture as well as of its content.

Psychoanalysis may have appealed to anthropologists because, as my colleague Gordon Allport has suggested, it is the most effortless type of psychology to lean upon. However, I think we must dig a little deeper than that. For all of the extravagant dogmatism and pretentious impressionism of much psychoanalytic writing, the anthropologist sensed that here at last he was getting what he had long been demanding from academic psychology: a theory of raw human nature. The basic assumptions of the theory might turn out to be false in general or in detail. The anthropologist was positive that the theory was culture-bound to an important degree, though the evidence of the past twenty years indicates that many anthropologists exaggerated the extent of the distortion they thought produced by bourgeois Viennese culture and by late nineteenth century science. At all events, psychoanalysis provided anthropology with a general theory that was susceptible of cross-cultural testing by empirical means and with clues that might be investigated as to the psychological causes of cultural phenomena.

Moreover, there were experiential factors that drew the psychoanalysts and the anthropologists together. Psychiatrists of all faiths were showing that there was meaning in the most apparently chaotic and non-adaptive acts of the mentally ill. This struck an answering chord with the anthropologist, for he was engaged in demonstrating the fact that the seemingly bizarre patterns of non-Western cultures performed the same basic functions as did our familiar customs. The same amnesty that the psychoanalyst grants to incestuous dreams the anthropologist had learned to accede to strange cultures. That is, both insisted that the queerest behavior had significance in the economy of the individual or of the society. There was no implication of moral approval, necessarily, on the part of either psychiatrist or anthropologist. Both merely agreed that behavior regarded as deviant could not be legislated out of existence unless psychologically satisfying and socially acceptable substitutes were discovered. The essential scientific task was that of gaining maximal understanding of underlying determinants.

Finally, the dominant experience of anthropologists had been as "unscientific"—in the narrow sense of that term—as that of the psychoanalysts. Physical anthropologists had long used the conventional symbols of "science": apparatus, measurements, statistics, though it is only recently that they have been re-casting their postulates and techniques to accord with twentieth century genetics, experimental embryology, and physiology. Anthropological linguistics has a methodology that is probably more rigorous than that of any study that touches human behavior with the possible exceptions of physiology and genetics. Cultural anthropology and archaeology, however, operate by procedures that are in some sense "clinical." Most cultural anthropologists are as innocent of statistics as are psychoanalysts. Ordinarily the anthropologist working under field conditions has as little possibility for control experiments as has the psychoanalyst who sees his subject an hour a day in the consulting room. The skilled of both professions do make predictions of a crude order and test them by subsequent observation. But these observations do not lend themselves to presentation to scientific colleagues in neat graphs and analyses of "t" distributions. Indeed both groups would maintain, without disparaging the indispensable importance of the quantitative for certain scientific purposes, that other scientific questions involve matters of form, position, arrangement that can never be settled by measurement. In linguistics and in fact probably in genetics, the crucial issue is not that of size or frequency but that of what point in what pattern. The principle of the circle does not depend upon measurement as such but upon a fixed patterning, even though measurements are necessary to draw any particular circle to specification.

And so the anthropologist, however skeptical he

might be of certain psychoanalytic dogmas, tended to feel in some measure at home in psychoanalytic psychology. He recognized certain similarities which confronted him in describing and interpreting a culture with those met by the psychoanalyst in diagnosing a personality: the relationships between forms and meanings, between content and organization, between stability and change. To be sure, this is also often true between anthropologists and non-analytic psychiatrists, personality and clinical psychologists. Indeed it is an induction from my experience that these groups tend to understand each other quite readily in spite of the superficially obvious differences in their aims and methods. On the other hand, anthropologists, social psychologists, and sociologists, although ostensibly interested in much more similar issues, appear to suffer frequently from bad communication. I think they are temperamentally much less akin. In field work I have been associated with social and experimental psychologists and also with psychoanalysts, psychiatrists, and clinical psychologists. The former were manifestly uncomfortable in every case. They found the uncontrolled variables of field conditions very disturbing. The latter group adjusted much more flexibly to unpredicted irregularities in interview situations, to the necessity for extemporizing with which every anthropologist is so familiar.

Of course, I should be inclined to attribute a good deal of the compatibility of anthropologists with psychiatrists and personality, clinical, and child psychologists to the vitalizing influence which psychoanalysis has had upon general psychiatry and these branches of psychology. Just as the anthropologist attempts to get a picture of the whole of a culture, so psychologists are more and more attempting to envisage the whole of a personality. In both cases this entails, for the time being at least, some deficiency in workmanship as well as loss of rigor. The anthropologist cannot have enough specialized knowledge to describe music, basketry, and kinship with equal expertness. Nor can the psychologist be equally well trained in mental and projective tests, depth interviewing, and techniques of the personal document. Nevertheless holistic, controlled impressionism has certain merits, at any rate for heuristic purposes in this particular stage of the development of the human sciences.

This retrospective discussion has led us to the present. Let me try to summarize briefly and, I am sure, spottily the present picture. The question I shall try to answer is: what do anthropologists think they are learning from psychologists that is useful to anthropology. I necessarily view the scene from an angle limited by my own knowledge and experience.

In the abstract, one would hope for fruitful collaboration between physical anthropology and physiological psychology. I have never heard of any, To most anthropologists, physiological psychology is seen from a distance as impressively detailed and workmanlike, enormously useful in practical ways during the war, self-contained, preoccupied, aloof except for some indications of willingness to submit passively to physiology. One would think that there was still a vast fruitful area for psychologists of perception and cultural anthropologists to explore, but both parties appear to have lost interest.

The lusty infant, constitutional anthropology, has had some productive contact with psychology, primarily medical psychology. Hooton and Overholser, the psychiatrist, collaborated in Hooton's study of criminals. As is well known, W. H. Sheldon has worked with and influenced anthropologists in the investigation of somatotypes.

Both physical and cultural anthropology have drawn significantly from comparative psychology. Two sources deserve special mention. The notable studies of Yerkes and his associates have left their mark on thinking in general anthropology. The brilliant researches of C. R. Carpenter have had a considerable influence upon theory in social anthropology.

Both physical and cultural anthropology are likewise concerned—in different ways—with problems of race and race relations. Here psychology has supplied the useful tool of mental and projective tests, which, even in their failures, have helped to show the interdependence of biological and cultural factors in this troubling area. However, the conceptual contributions of psychology have been smaller than might have been anticipated. There are two notable exceptions, Gordon Allport and Otto Klineberg. Allport's studies of group prejudice have deeply affected anthropological research. Klineberg has both taken much from anthropology and given much in return.

It is extraordinary how little influence most of those usually classified as social psychologists have had upon anthropological thinking in contrast to their great influence upon sociology. I suspect this must be understood in terms of temperamental selectivity and of anthropological prejudice against

sociologists. This impasse is perhaps most strikingly evident in studies of language.

During the last ten years there has been a fruitful rapprochement between learning theory and anthropology. In particular, the group from the Yale Institute of Human Relations (Dollard, Mowrer, Miller, and Sears) have stimulated important work by Whiting, Ford, Davis, and others. Certainly there is almost nothing which psychology could give anthropology that would be more useful than a sound theory of learning and effective methods for studying learning. Some of our most central problems (the culturalization of the child, diffusion, resistance and susceptibility to culture change) lie in this area. Miller and Dollard made a promising beginning in *Social Learning and Imitation*, but this important book has some of the defects that almost always inhere in pioneer efforts.

It is noteworthy that in the so-called "culture and personality" field the basic bridges have been laid down by medical psychologists. However, anthropologists are very grateful for the rich ideas and facts provided by such psychologists as Dennis, Havighurst, and Anastasi. More recently, a few clinical psychologists, such as Sylvan Tomkins and William Henry, have assisted in the solution of anthropological problems. A closer relationship between developmental psychologists and both physical and social anthropologists is much to be desired. Margaret Mead has recently discussed the Gesell-Ilg theory of maturation in the *American Anthropologist*.

At the moment there are encouraging signs. The applied psychologists and the applied anthropologists are beginning to get together in the personnel and industrial specialties. Rapaport and other psychologists are providing vitally necessary researches on remembering and forgetting. The studies by Sherif, Bruner, and others on social factors in perception are pointing the way to the understanding of the varied spatial and temporal orientations which characterize different cultures. Some anthropologists see in Lewin's field theory a possible framework for the creation of a basic social science which would unify much of psychology, psychiatry, sociology, and anthropology.

This question of new alignments in the disciplines, of a re-slicing of the pie, is a difficult one on which my own opinions have not yet fully crystallized. Academic people undoubtedly are traditionalists and tend to be irrationally resistive to structuring scientific territories in accord with new frontiers arising from the development of actual investigations. On the other hand, I see weighty arguments against divorcing the biological from the other aspects of psychology and anthropology and marrying the relict to sociology. I am firmly convinced that every kind of psychologist needs a substantial training in biology, and the same is true for the anthropologist. Indeed the firmest link between psychology and anthropology has been the fact that they alone of all the so-called social sciences bridged the gap between the organic and the socio-cultural. This has, I think, made for tough-mindedness and for realization of the full complexity of behavioral phenomena. Although some social psychologists and some cultural anthropologists have tended too much toward a complete environmentalism, still the main currents in both professions have steadily insisted upon the relevance of genes, biological structure and process, maturation and other organic facts. If regroupings can be made which provide in training and professional canons for the preservation of this postulate of the inevitable biological basis of human nature, then I am heartily in favor of experimental new departments.

We have given you the concept of culture which is now part of psychological thinking. We ask from you in return a formulation of the human nature which is the raw stuff that all cultures act upon. Both psychology and anthropology could well begin to lay as much stress upon the similarities in all human beings as they have, quite properly, upon the differences. The anthropologist, looking at psychology, must express some fear lest "human nature" go the way "mind" has gone.

There are many things for us to do together. We need your technical skills, especially in culture and personality work; we need your collaboration in the improvement of interviewing techniques and in gathering and analyzing personal documents; we need a pooling of our different kinds of knowledge of human biology. I hope that for at least a time we can do our joint researches within the framework of a conceptual scheme which makes explicit some of the postulates we hold in common but which also gives free play to the legitimate and healthy variations in intent and method. In Kroeber's wise words:

> "But precisely if psychology and anthropology are to cooperate, it seems that they should recognize and tolerate each other's individuality."

MEASURES OF SCIENTIFIC MERIT IN APPLIED PSYCHOLOGY

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LET me present here a plan and plea for the extension of scientific methods in all branches of applied psychology and in education, sociology, medicine, economics—in short, all the efforts to make a scientific study of human experience and behavior as such.

In my thirty-two years of the graduate deanship I have viseed more than 2,000 proposals for doctoral dissertations and more than 10,000 masters' theses by signing my name in approval or disapproval. In the same period it was my duty to appraise the research work of members of the staff in the entire university. My conscience and my colleagues taunted me for acting as if I were competent to appraise research in all fields of knowledge. Let me say at once that I maintained no such pretense, but the work had to be done and I was aided by consultation with the departments concerned.

Now, looking back, I have analyzed the procedure and find that there are certain principles of research which are applicable directly or indirectly, in whole or in part, to all fields of research and creative work. I base this conviction on three lines of observation: first, the established scientific principles in the physical sciences; second, my adaptation of these to the problems under my direction in the psychological laboratory; and third, the universal struggle in the applied sciences and arts for scientific techniques in the pursuit of their problems.

When a student plans a choice of a topic for experimentation, when a professor discusses the value and legitimacy of such an experiment, and when a reader evaluates the contribution in a journal, each may well apply as a measuring scale some established principles of scientific merit by asking himself the following nine questions.

1. Has the experimenter fractionated his problem so that he can deal with one specific factor at a time and set up an operational definition?

2. Has he isolated the chosen factor for experimentation so that it can be varied under control?

3. Are his findings recordable, countable, and repeatable for verification?

4. Has he kept other factors, subjective and objective, constant?

5. Is the factor measured undistorted by isolation from the total situation, subjective and environmental?

6. Is the factor measured significant?

7. Is the statistical method involved, if any, sound?

8. Does it contribute to some fact that utilizes or has a bearing on systematic science?

9. Are the conclusions after each measurement limited to the role of the factor measured?

Each of these sanctions is a test in itself. When understood, they would seem but common sense applied. Failure on one or two may invalidate the whole project. A numerical rating scale may be set up by assigning merit of zero to five on each item where zero means no science and five means full compliance with the scientific sanctions. In applied psychology one can never earn a rating of straight five on account of the inevitable variability in human factors. The same is true of general biology, though not quite to the same extent, but good work in this area should rate somewhere between four and five.

These are the sanctions which we have used with surprising consistency in the entire field of scientific approaches to music and the fine arts. My argument here is that if these sanctions can be applied to the study of music and the fine arts, which is a domain usually very abstruse and often esoteric, that is proof that they can be extended to far more forms of experience and behavior than is now recognized in the social sciences and education. If this were done, there would be a radical revolution in the social sciences.

To show that these sanctions or their equivalents can be applied more broadly than they are at present in these fields, let me make some simple

statements of fact relative to each of the nine sanctions.

1. Far more problems in these fields than is at present recognized can be fractionated so as to isolate and define points of attack for scientific measurement. Witness the problems of learning, personnel, democratic living, mental economies, charities, and crime.

2. Most of the problems in these fields in which the situation is observable by eye or ear can be faithfully recorded for scientific analysis and segregated for treatment. Witness the introduction of measuring instruments such as the wire recorder, motion pictures, eye-movement cameras, and all the types of specific problem recorders in experiments.

3. Records of achievement in an experiment can be made by the same type of instruments that are used in the recording of the original act to be studied provided 1 and 2 above are satisfied. Witness the use of standard instruments of precision in building a recording instrument for a given specific act and the various types of achievement tests for which we now have statistical and theoretical techniques.

4. Skill and insight of the experimenter for the control of significant accessory subjective and objective variables present can be acquired by basic training. Witness the necessity of training in the physical and biological sciences and especially rigid training in the psychological laboratory before undertaking complicated practical problems.

5. The distortion by isolation from the total situation can be avoided by drawing the experimental material or stimuli from the complete record of the original situation. Witness the possibility of fractionating hundreds of items in a single phonophotograph of a song or from a motion picture of a football game.

6. The significance of an experiment can be judged by common sense and logical evaluation of the hypothesis involved. Witness the similarity to evaluation in physiological experiments which always work toward a progressively clarified purpose.

7. Validity and reliability of statistical treatments can be attained under the above conditions. Witness what saving could be made today by requiring that statistical data should be countable.

8. The merit of experiments is measured by the extent to which they contribute basic and enduring facts toward systematic science. Witness the motivational value of having scientific merit as a goal, even in mere observation of incidental behavior situations, in teaching, in popular writing, and in the disciplines which are not called scientific, such as history, literature, and art.

9. The conclusions from experiments in these fields must be logically restricted, even at the expense of limited practical value. Witness the frequency of experiments which are thoroughly sound in themselves but are assigned false implications in the interest of service.

When we turn to the year's output in applied psychology and social science in general, as represented by thesis or dissertation topics, journals for the year, or the programs for the annual meetings, we find a part of the material heartening and another part, often the larger, disheartening. It is heartening to see the very large number of competent investigators working with scientific techniques where there were none before. Gratifying progress has been made within these fields in the present century.

On the other hand we are confronted with reports which reveal little or no grasp of scientific procedure. It is to this group of so-called researchers that I am making an appeal. I would not flaunt the slogan of science. Very little of our useful knowledge is strictly scientific. Within the fields under consideration there are many problems which should not be attempted scientifically. There are many non-scientific problems which can be undertaken only with scientific experience as in more or less controlled observations. Others can be said to be scientific only in principle, even when they come from scientifically trained men working in an undeveloped field. But where there is a claim of science we must ask our nine commonsense questions before accepting or rejecting the findings.

There are some reasons for lack of scientific merit in these fields. The chief cause for failure lies in the lack of willingness to sacrifice. There are two general attitudes in the social sciences: the omnibus theory and the specific theory. On the omnibus theory the experimenter aims to deliver a whole solution of the problem directly, and this may be all that he wants, but the experiment will have little, if any, scientific merit. On the specific theory the experimenter is willing to sacrifice by dealing with one definable element of the situation at a time with all that involves. In publications on science in music there are perhaps a hundred followers of the omnibus theory to one following the specific theory, and that is perhaps typical of related applied science. The omnibus theorist asks, for example, is

this child musical? The specific theorist asks, in what respects is he musical and to what degree?

Another cause is failure in orientation. Before an experiment can be set reliably, a study must be made of the history of known facts in the total situation and of all the steps that have been taken by other experimenters in solution of the problem, leading to a logical insight which results in a hypothesis. It is not necessary that the experimenter should be able to foresee the results of the hypothesis, but there must be a reasonable justification for the experiment. In many situations the first step should be an experimental factor analysis.

Lack of training in, or knowledge of, the basic sciences is another cause of failure. In the applied sciences the object is generally the solution of a practical problem, and the young investigator, particularly in education and sociology, tends to go right after the whole solution without training in scientific techniques and skills. Failure to require this training has held back the social sciences tragically. It is of course evident that lack of time and facilities for proceeding by a scientific method is one of the commonest alibis. Among students, the lack of efficient direction and the lack of entering into the spirit of teamwork with the past and present investigators in the field is a frequent cause of failure.

One of the objectives in basic scientific training is the development of the power of scientific integrity; that is, the willingness to apply cold logic to the problem or data in hand. This applies especially to the interpretation of experimental findings. The absence of scientific attitude, the love of science, the emotional glow which comes from persistent pursuit of truth, and companionship in the field of scholars are common causes of failure. I have coined the word, euphany, with the adjective, euphanous, which may be defined as denoting a deliberate and adequate statement of fact. Lack of this ability is a common frailty among researchers.

Briefly, then, my message is this: You can be scientific! Prepare yourself in techniques, background, and theory, and enter into companionship with those who are willing and able to make the necessary sacrifices and rejoice in the pursuit of verifiable facts!

INSTINCT: A COMPOSITE STUDENT VIEW

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A PERSISTENT problem in the teaching of psychology is the use and misuse (from the psychologist's standpoint) of common, everyday words. No doubt most instructors, realizing this, make a conscientious effort to define terms and to stay within the bounds of their precise terminology. Unfortunately, our definitions frequently misfire because we do not really know the everyday use of the terms. The reasons are various: because we have thought for a long time in technical ways, we forget the popular usage; or we may be so impatient with common sense that we simply override it; or we take for granted the acceptance of certain premises. Any of these attitudes has harmful repercussions; perhaps the greatest of these is the confusion engendered or reinforced in the mind of the hearer. The wise student, therefore, may simply decide that the best policy is agreement with the professor to avoid argument—and to get a good grade—but, privately, he can go along just as before. He may have learned something about how to win friends and influence people, but it is doubtful that he has learned much psychology.

A specialized jargon, of course, is not psychology. But the definition of the facts of behavior in objective terms is at least part of it. The term "instinct" has proven to be one of the most tenacious and troublesome of those concepts with which the teacher of psychology must contend. The trouble is more deeply rooted than the mere fact that it has universal popular usage. For one thing, the term is employed in many other academic fields, such as biology and all of the social sciences, so that the student enters a psychology lecture hall heavily laden with classroom experience with the word. For another thing, it is difficult, if not impossible, to teach psychology without considering the phenomena commonly attributed to "instinct." And it is really unsatisfactory to reject the word, following the exhortations of the behaviorists, or to substitute some "less controversial" term. To adopt either practice involves a tedious rationalization on the part of the lecturer, and probably does little to clarify the matter for the student. Furthermore, either policy seems hardly necessary, in view of the fact that perfectly legitimate data are on hand to give an adequate meaning to the concept (see, for example, *1* and *4*). In any event, if the student has confusions, why not deal with them?

After struggling with this problem before his classes, the writer decided to inquire concerning the sort of preconceptions students have about "instinct." At the start of the semester, long before the matter came up in the lecture, the class was given the assignment to think over what they understood an "instinct" to be, and to bring a written statement to the next class meeting. They were told not to refer to any textbook or other source in formulating their answers. To be sure, some students may have disobeyed this injunction; if so, the results become even more startling. Nearly the entire class returned answers, varying from a few words to two pages. The summary which follows is based upon the definitions of 113 students in the beginning class.[1] These statements are spontaneous, rather than based upon a standardized questionnaire, hence do not necessarily represent the true incidence of opinion on all points.

Classification of Responses. As a preliminary step, each statement was examined for the terms it contained, and all of these listed separately, except for quite obvious synonyms (e.g., "inherent," "inborn," and "innate" for *hereditary*.) Thereafter, broader groupings were made, combining words or phrases which, in the judgment of the writer, reflected the same basic characteristics. Three fundamental features of the answers ultimately emerged, namely, A, the general functional nature of instinct, B, specific characteristics of (an) instinct, and C, examples of instincts. Each of these will be considered in turn.

[1] Since the class is typical of the University of Hawaii, the students represent a highly varied racial background, Japanese, Chinese, Caucasian, Filipino, Hawaiian, and even part-Samoan. However, they are all products of either mainland or territorial schools. Cultural factors may, thus, make this sample of answers different from one obtained elsewhere.

A. THE GENERAL FUNCTIONAL NATURE OF INSTINCT

Student opinions assigned instinct to one of four categories, most students restricting it to one of the categories only, but a few placing it in more than one. These categories follow with the incidence of citation:

1. *Impulse* (32%). About one-third of the responses defined "instinct" as some internal motivating force. Representative statements are: "natural impulse" (or tendency or force); "sort of urge"; "something of an impulse which is spontaneous and has nothing real to back it up"; "motive without thought"; "driving force"; "tendency to act"; "subconscious force"; and "different facets of a single force" (stated as "will to live").

2. *Response or activity* (33%). Another third of the definitions placed instinct on the side of what the organism does, rather than what brings the response about. Typical statements are: "natural act"; "physical response without thought"; "sudden response" (to some imminent danger); "sort of habit"; "reflex"; "natural and unplanned behavior"; "immediate response to stimuli"; "automatic reflex or action"; "unconscious doing"; "things we do for the first time, we usually do instinctively"; etc.

3. *Mysterious knowledge or intelligence* (32%). A little less than a third said that instinct is some peculiar or supernatural characteristic. Thus, it was described very often as a "sixth sense" or as "intuition." Others defined it as "premonition"; "a fact which becomes known without any past knowledge"; "something beyond the natural"; "prime ability—reaction without stimulus, whereas reflex is an answer to any stimulus"; "God-given intelligence"; etc.

4. *Sensory phenomenon* (4%). If "sixth-sense" be excluded, there is still a small number of students who define instinct in terms of quite definite sensory function. Thus, two students literally ascribed instinct to feeling, seeing, hearing, tasting, and smelling. Others linked it less specifically to the sense organ.

B. SPECIFIC CHARACTERISTICS OF INSTINCT

Although a great variety of terms was used, they can be summarized as synonyms or aspects of 12 ideas. The figures shown are percentages of students (hence, will add to more than 100), and are merely suggestive of relative emphasis, as noted above.

1. *Hereditary* (39%). Clearly, the salient feature of instinct to the students in this sample is that animals and/or men are born with it.

2. *Unlearned* (23%). This characteristic has been listed separately from the foregoing one, at the expense of stretching a point. The term "unlearned" covers numerous statements which did not mention heredity, as such, but implied behavior which is independent of prior experience, or, more importantly, behavior which is more generalized and less rigid in a species than was apparently meant by *hereditary*. It may be taken, perhaps, as a more sophisticated opinion than the simple statement that instinct is innate.

3. *Unthinking* (18%). This characteristic covers instances where instinct was differentiated from rational behavior. It was said not to involve reasoning, to be blind, etc.

4. *Unconscious* (16%). Typical statements here were "unconscious preparation," "part of subconscious mind," "working of subconscious mind," "unconscious doing," etc.

5. *Automatic* (12%). Instinct was said to be infallible, involuntary, mechanical, not subject to self-control, automatic, etc.

6. *Spontaneous* (11%). Instinct was sometimes described as being unplanned, unpremeditated, unrehearsed, sudden, etc.

7. *Present more in animals than in men* (9%). A surprisingly small number of students differentiated between human beings and animals, in the number and/or potency of instincts.

8. *Involves learning and/or cultural factors* (8%). A substantial number of students stressed the role of experience in the formation of instincts. (The fact that practically all of these students had had anthropology in their freshman year no doubt contributes to this factor.) In some of these definitions, an instinct was said to be an act trained to perfection, until it has become automatic (e.g., the footwork of a boxer).

9. *Universal* (7%). Although the universality of an instinct (at least for a given species) may have been taken for granted by many students, it was specifically cited as a criterion by comparatively few. Four varieties of generality were expressed, as follows: (a) universal in an unqualified degree; (b) universal, but different degrees are present in different individuals; (c) universal in a given species, but different species have different instincts; and (d) universal in a given (human) cultural group, but different cultural groups have different instincts

(these are said to be inherited cultural traits, and unlearned).

10. *Survival* (6%). A few students stated that instincts serve the organism by assuring its survival.

11. *Unvarying* (3%). It is surprising that so few students stressed the inflexibility of instinct. Although the same idea may be implied in some of the above categories, it may be that many of these students have been taught that organisms can vary their behavior in adapting to the environment.

12. *Resembles conscience* (2%). Instinct was said by two students, respectively, to be the "sense of right or wrong" and to help in distinguishing right from wrong.

C. EXAMPLES OF "INSTINCT" CITED

Not all students gave illustrations of their definitions. Those who did offered an amazing variety of behavior, both animal and human. The following rough classification will summarize this feature of the statements.

1. *Response-patterns.* The commonest example was bird-migration, followed by egg-laying behavior of insects (especially the wasp), web-spinning of spiders, sudden closing of the eyes when some object (notably a fly) passes near them, crying of a baby, swimming (in puppies), and the sucking-response of infants. Others were nest-building (birds), child running to its mother from danger, screaming when frightened, salmon swimming upstream to spawn, reaching for toppling object, maintaining balance, birds teaching their young to fly, obtaining food, reaching and holding in infants, child walking, and flying of birds.

2. *General "propensities."* The most frequently cited in this category was self-protection, with protection of young by the mother next. Others were gregariousness, imitation, inquisitiveness, homing (pigeons), food-acquiring, group living, "mother instinct" and parental love, etc.

3. *Intuition.* Some examples of this sort were premonition of danger, warning of impending danger, warning of imminent contact, and "wife knowing when I am lying."

4. *Organic drives.* Specifically mentioned were sex, reproduction, hunger, and eating.

5. *Emotions.* Regarded as instincts were pity and helping others, fear, hate, jealousy, love, and "natural enemy sense."

6. *Physiological functions.* Sleeping and shivering from the cold were cited.

7. *Life-impulse.* In a final category were "instinct to live" and "will to live."

CONFUSIONS ABOUT "INSTINCT"

The preceding summary affords insight into the kinds of conflicting ideas brought into the classroom with regard to the particular concept of instinct. A few students frankly expressed uncertainty or outright confusion. It is impossible to say with what degree of assurance the remainder set down their views. In any event, it is possible to formulate some of the major problems.

The first may be stated as the *confusion between action and impulse.* En masse, students do not know whether an instinct is what the organism does in a certain situation, or what makes him do it. This confusion is by no means confined to students, as Dashiell has pointed out (2, p. 116), (and see also English's careful discussion (3, p. 67)). An examination of textbooks now in widespread use will further bear out this confusion. It behooves the teacher of psychology to take cognizance of this conflict and attempt to resolve it convincingly.

The second difficulty may be formulated as the *confusion of heredity.* The student does not have any clear idea of the extent to which, if any, instinct is inherent in the organism and is transmitted from one generation to the next. A related problem is the universality of instinct—is it a phenomenon common to life? is it different in different species? Do men have "instincts" as well as animals? Perhaps some of these doubts are cleared up during lectures on heredity and environment, since, in part, they reflect ignorance of the mechanisms of heredity. On the other hand, even a discussion of genes and chromosomes may do nothing to relieve the vexatious question of what instinct *is.* Instinct as a "mysterious something" must, the writer believes, be succinctly dealt with by the teacher with this in mind.

A third problem is the *confusion of learned vs. unlearned behavior.* The student appears to have only the vaguest idea of the degree to which instincts are learned, automatized responses, or are independent of experience. He does not know whether there is a difference between reflex and instinct. If he believes instincts to be innate, he does not know whether or not they can be modified by experience, or to what degree they are (or become) subject to self-control.

Perhaps the greatest confusion of all (and the crux

of the matter) is familiar to psychologists in many different contexts; namely, the *confusion of mystery*. Instincts in the mind of the beginning student are intangible and wonderful, vitally important in the scheme of life, but elusive. We see them in operation, but there doesn't seem to be any understandable basis for them. In short, they are "mysterious somethings," apart from organic facts like bones and skin, nerve-cells and muscles, viscera and the germ plasm.

Hence, one cannot lightly disregard the term instinct. Perhaps it is unfortunate that it is still with us. It would be much more pleasant if, depending upon our separate penchants, we could either assume a *tabula rasa* in the beginning student, or feel confident that, as the only sound one, our theory would vastly outweigh the nonsense students pick up elsewhere. But neither of these conditions very often obtains in a psychology classroom.

This survey of such a classroom indicates that it is worth the effort to discuss instinct at some length, taking account of the probable cross-section of diverse preconception present among the students. An alternative explanation or redefinition cannot be developed with much success without first accounting for the points brought out above. Mysteries and facts make poor companions.

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Comment

Essential Courses for Undergraduates

To the Editor:

We have recently sent check lists of undergraduate courses in psychology to the chairmen of twenty-eight leading departments of psychology, asking each to indicate the courses he considers essential or desirable as background for the graduate study of psychology. Replies were received from twenty-five. The results are given in tabular form.

In addition to the courses of the check list, the following courses were suggested by one or more chairmen as essential: biology, physics, introductory philosophy, trigonometry, analytic geometry, logic, scientific method, and social science. Other chairmen suggested the following courses as desirable but not necessary: a survey of trigonometry, algebra and calculus, zoology, personality, scientific method and logic.

Several department chairmen commented that the student expecting to do graduate work in psychology could spend his undergraduate time more valuably in securing background preparation in the sciences than in taking a large number of undergraduate psychology courses.

It does not, therefore, follow from the above list that every department chairman would want each undergraduate student to take all of the courses in the "considered-essential" column. Nevertheless, students as well as faculty members should find this table useful in selecting the most generally approved list of undergraduate courses.

A tabulation showing the number of times each course was checked as essential or desirable

COURSES	CONSIDERED ESSENTIAL	DESIRABLE BUT NOT NECESSARY
General Psychology	25	—
Experimental Psychology	22	3
Elementary Statistics	16	8
College Algebra	15	8
Heredity	12	10
Psychological Measurements	12	8
Abnormal Psychology	8	11
Systematic Psychology	8	11
Social Psychology	8	10
Child Psychology	6	13
Physiology of the Human Body	5	16
Applied Psychology	5	11
French *and* German	5	1
French *or* German	4	3
Calculus	1	17
Educational Psychology	1	8
German	1	1
Mental Hygiene	—	16

IRVINE M. DUNGAN and GERTRUDE S. EKAS
Berea College

Psychology and Clinical Psychology: A Reply

To the Editor:

The June 1948 issue of the AMERICAN PSYCHOLOGIST carried an editorial in the *Across the Secretary's Desk* column that is worthy of further comment for two major reasons: an editorial in the professional organ of the APA tends to be construed as comparable to an edict of policy. Secondly, the problem considered in this editorial is of high import, inasmuch as it is an analysis of the official statement of an important government agency that is pioneering in the utilization of psychologists in medicine. The voicing of opinion by the higher echelons in the profession has far reaching consequences and therefore, such statements which may induce policy, should be more closely examined and weighed prior to public expression.

The objectionable features in the editorial might be summarized under two categories: One, the editorial dealt in a dogmatic, final manner with a highly controversial problem, namely, whether psychology at present is obligated to train the people who apply the science as practitioners rather than as pure scientists. Incidently it is very interesting that in the same issue of the Journal the report of the Policy and Planning Board of the APA was considerably more guarded and thoughtful in considering the same question. Two, the question of the organizational structure of the science of psychology tends to become obscured when non-essential considerations are used as a basis of evaluation.

The editorial argues that the goal and focus of the sciences of biology and medicine differ as do those of psychology, as a pure science, and clinical psychology, as an applied science. However, this writer believes that the conclusion drawn in the editorial regarding the feasibility of separating these two sciences is thinly bolstered, if the argument regarding the "profound ignorance" of these two sciences is offered as justification for continued union. If one were to see the goals and purposes of psychology as narrowly and limited as presented in the editorial, it would seem to be more logical to conclude that a separation of the sciences would be beneficial to the fullest development of each,

since confusion and cross purposes alone obtain from their unification.

Humanity needs both the researcher who probes the pure science and those who apply this science. Both groups have important functions and roles within our society. One is not more or less important than the other. Dr. Lawrence S. Kubie, Associate Neurologist, Columbia University College of Physicians and Surgeons, summarizes this point of view very succinctly:

> "There is no profession, no scientific discipline every member of which can or should be created in a research sense. No science can live on research alone in an ivory tower out of touch with the challenge of practical application. Actually science always makes progress by the interaction between the pure and the applied sciences in the field. We constantly check the value and validity of our pure scientific concepts by their applicability. The interplay between the two is an essential and healthy part of training and of the maturation of any science" (*Transactions of the First Conference on Training in Clinical Psychology*, p. 2).

The supposedly philosophical "cold" viewpoint, stated in the editorial, that "it is more beneficial to society for a psychologist to make a research contribution to our knowledge of human behavior than it is for him to help an individual patient" is an unwarranted generalization. Such thinking is not considered to be meaningful by those individuals who work with suffering human beings either as physicians, clinical psychologists, or social workers. Furthermore, the sentiments expressed in the editorial are by no means the only agreed course or true alternative for future action by the body of psychology. Both research and applied activities can be maintained independently without detriment to one or the other.

The editorial speaks of "pressures" from clinical psychology, and the "disastrous" affects of letting solutions of problems pertinent to clinical psychology interfere with the progress of psychology. Such a conceptualization of the problem is highly emotionalized, and presents a restricted point of view. It fails to give proper weight to the facts and reality status of the total problem. The begrudging acknowledgment of the needs of clinical psychology is made in poor taste to those whom the editor describes as "too long a neglected minority within psychology." This narrow viewpoint can be very damaging to the body of clinical psychology, especially when it is espoused by one who by virtue of his position exerts great influence in resolving basic problems in the field.

These opinions are not only those of the writer, but are also those of many clinical psychologists who express exception to the point of view contained in the editorial.

ARTHUR TEICHER
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Are Our Silent Presuppositions About Prejudices Correct?

To the Editor:

The discussions about the role of prejudices in intergroup relations are getting more and more confused (see, for instance, the Round Table discussion of The University of Chicago, of May 2, 1948 on *What do we know about prejudice?*) They often reveal more about the bias of the particular social scientist than about the attitudes of the common man who is allegedly the object of investigation. I wonder, therefore, whether it would not be a good idea, instead of continuing our futile discussions, to make a new start by considering the following aspects of the situation:

1. The crisis of our age is not restricted to the social world itself. It involves also the mirror of this sick world—the equally pretty sick social science. What we really need—I agree in this respect fully with the sociology of knowledge—is not more "research" about "facts" but a *critique of our silent (hidden) presuppositions*, the validity of which is very much in doubt.

2. The current theory of prejudices is based on a whole series of presuppositions which, if made explicit, turn out to be *obviously false*. They are accepted only because they are not made explicit. The real thing in inter-racial and inter-cultural tensions is not that people are *prejudiced* but that they are *different* in various respects and for various reasons. The stereotyped conceptions which members of different groups have about each other are an essential part of their being different. Not *prejudices* but *differences* are at the bottom of tensions, dilemmas, and difficulties in intergroup relations. Everything else is secondary in nature.

3. There is only one effective therapy in intergroup relations: to admit and to discuss frankly the existing differences, at the same time explaining that there is nothing wrong with being different. Since we are different, we can only understand each other if we are aware that we are different. This, to my mind, should be the basis of an intelligent intercultural and interracial education. For denying differences which are actually there means making propaganda for misunderstanding each other. And the inevitable result of this is, in collective terms, an increase of intergroup tensions—for the concealed and denied differences remain, and are more disturbing than if they were openly admitted. And in individual terms, the result is neurosis which develops if we conceal, or deny, what we really are.

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Chicago, Illinois

Across the Secretary's Desk

WHO RUNS THE APA?

A lot of people do. The APA with all its boards and committees and with its 18 divisions and all their boards and committees requires many people to keep all of its activities running smoothly. When the November, 1947 AMERICAN PSYCHOLOGIST appeared with its long list of officers and committee members, E. G. Boring counted the people involved and the number of positions which each held. The list was incomplete because division committees were not included. Even so, 172 people were named. This year we have made a similar count of 209 people whose names will be included in the list of officers and committees to be published in next month's AMERICAN PSYCHOLOGIST. The number of members holding each number of positions in 1947 and 1948 is:

Number of positions held

	1	2	3	4	5	6
1947	111	36	20	2	2	1
1948	129	50	25	4	1	0

To have a large number of members actively participating in the Association's affairs means that APA policies are shaped by the combined judgment of many people. They represent different psychological interests, different age groups, different parts of the country, and different kinds of employment. This spread of responsibility also means that many people are experiencing the satisfaction of helping to manage the APA's affairs and are gaining experience that will prepare them for more important future positions in the Association's management.

When I became a member of the APA in 1931 it seemed to me that the Association was run by a little clique who sat on the Council of Directors, made and seconded motions in the annual business meeting, and spoke whenever any topic came up for discussion. I have heard the same charge made every year since—that the APA is run by a small clique. In a sense, of course, the statement has always been true; at any one time there is always a small group of officers and directors who have primary responsibility for running things. But going back over the list of officers and directors—as R. S. Woodworth did a dozen years ago and as I have now done again—quickly shows that the small group has always been a constantly changing one. For example, no member of the present Board of Directors has served for more than three consecutive years and only one was on the Board five years ago. This amount of overlap is usual. If the members of the Board of Directors and the officers of any one year are compared with those of five years earlier there is usually an overlap of one, or two, or occasionally three members. (The Executive Secretary now provides some continuity. He does not vote, however, and his term of appointment is limited by the By-Laws.)

Now, with Council members elected by 29 different constituencies (18 divisions, 9 geographical regions, the Conference of State Psychological Associations, and the Department of Psychology of the American Teachers Association), with division officers elected by 18 different constituencies, with the Board of Directors and most APA committee members elected by the Council, and with the President and President-Elect chosen by the entire membership, there is no possibility of management by a small self-perpetuating group.

In selecting people for the APA's many official positions, attention should obviously be given to the characteristics which each job demands. A division secretary, for example, must have the habit of answering his mail promptly. If he does not, his desk will soon be piled with complaints and the whole division will suffer. A member of the Council of Representatives, as a second example, must have the habit of attending APA meetings regularly, for to fulfill his functions he must attend Council meetings. Any division or region that elects a Representative who does not attend APA meetings regularly is forfeiting a vote on Association matters.

Election to any APA position carries responsibility, but the greatest amount of responsibility comes when one is elected to the Board of Directors. For, despite the many people who hold office, top management of the Association's affairs does rest in the hands of a small group—the four officers and the six directors. The Board of Directors meets twice a year. Interim business is conducted by mail. All told, each member probably gives some 10 to 12 days a year to his duties. In those days he makes decisions or recommendations concerning the management of an association of close to 6000 members, an annual meeting that will attract over 2000 persons, and a budget that is now close to a quarter of a

million dollars a year. I have been interested in watching the directors assume this responsibility. Personal wishes and biases are submerged in an effort to decide in terms of the welfare of the whole organization. Debated points get talked over and considered from all angles until agreement is reached. On a number of occasions I have seen a director point out the difficulties with and the arguments against a move which he personally was recommending.

The feeling of responsibility which characterizes the discussions of the Board may come in part from the trust which the members necessarily place in the directors. Partly, I suspect, it comes from the fact that officers and directors have usually served a considerable apprenticeship in APA affairs before attaining the top positions.

The Program Committee is an example of the APA's apprenticeship system. Its members have to decide how to arrange the many elements of the program to minimize the amount of conflict. They have to fit the program into the available time and space. They have to guess which of several concurrent sessions will draw the largest audience and so should be assigned the largest room. Membership on this committee lasts three years. In his first year, a member does some work, but mostly he learns what it is all about and what the problems are. In his second year he can contribute a good deal more. In his third year he serves as chairman and does much more work than he did in both his preceding years. But those years have trained him for the responsibilities of his chairmanship year.

Very rarely is one elected to a top office who has not gone through an apprenticeship of service on APA committees or in the committees and offices of APA divisions or other psychological associations. Ernest Hilgard, the present President, is a good example. He was a member of the National Research Council's Emergency Committee in Psychology and of its sub-committee on Survey and Planning. He has been a representative of the APA to the National Research Council. He was chairman of the committee that revised the constitution when the APA and the AAAP combined. For three years he was a member and for one year the chairman of the Policy and Planning Board. For two years he was a member of the Council of Representatives. For one year he was chairman of SPSSI. He can serve more effectively as the Association's President because during the six years in which he held these earlier positions he learned a great deal about the APA and its problems.

It takes time to acquire the prestige and to become well enough known to be elected to the Board. Most newly-elected members have been in their forties, a few in their fifties, still fewer in their thirties. The present members range in age from 36 to 56, with a median age of 46. As the APA grows larger it takes more time to demonstrate one's competence to the point where one is elected to these offices. Ten years ago the median age of officers and directors was 43 instead of the present 46. If the directors themselves were doing the electing, I suspect that they would choose a still younger group. The directors do pick out the APA's Secretary and Treasurer. Election is by the Council now and used to be by the annual business meeting, but the nominations are made by the Board of Directors. In the last 30 years the Secretaries and Treasurers have ranged in age from 29 to 46. Six of the 13 were under 35 and all but two were under 40 at the time of their election.

The Board of Directors has given considerable thought to the problem of finding younger people for membership on APA committees and boards. The Board has instructed me to enlist the help of divisions and state and regional associations in an active hunt for promising young psychologists who will work effectively on APA committees.

Local psychological associations and the APA divisions can help on this problem. Being smaller, their members know each other better. They have an opportunity to elect younger officers than the APA usually does. Like the APA, however, they can train their officers through committee appointments. Many of the future officers of the APA will come from the ranks of those who work satisfactorily in these positions, for the APA is run by a constantly changing group of members who have demonstrated their competence through such apprenticeships.

DAEL WOLFLE

Psychological Notes and News

Mail addressed to the eighteen APA members listed below has been undelivered because of obsolete addresses. If you know of more recent addresses please send them to the Office of the Executive Secretary, 1515 Massachusetts Avenue N.W., Washington 5, D. C.

Viola Caprez Ames
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% Lt. Eugene B. Brody
385th Station Hospital
APO 124, % P.M.
New York, N. Y.

Marjorie Fiske
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New York, N. Y.

Eleanor H. Hayes
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Mary Frances Reed
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Dr. Herbert Shuey
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Mrs. R. D. (Elizabeth C.) Strickland
2369 Williams St.
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Rachele M. Thomas
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New Haven, Connecticut

Mrs. Joseph A. Tyburdry
365 Marlborough St.
Boston 15, Mass.

H. T. Tyler
Sacramento College
Sacramento, California

Natalie Zinn
Worcester State Hospital
Worcester, Mass.

Henry S. Odbert has joined the Secretariat of the Committee on Human Resources of the Research and Development Board. He was formerly assistant professor at Dartmouth.

Clyde H. Coombs, on leave of absence for a year from the University of Michigan, will work at the Social Relations Laboratory of Harvard with Samuel Stouffer, Fred Mosteller and Louis Guttman on problems of scaling methodology.

Dorothy Park, formerly of Stephens College, is now head of the department of philosophy and psychology at Meredith College.

Alice E. Bell, formerly of Stephens, is now associate professor of psychology in the department of psychiatry at the University of Cincinnati.

Irene Robinson of the University of Chicago has become an assistant professor at the University of Utah.

Stanley B. Williams resigned as assistant professor at the Johns Hopkins University to become professor at William and Mary College.

William Schofield has been appointed as assistant professor in the Department of Psychiatry and Neurology of the Medical School of the University of Minnesota. His duties will include the supervision of graduate students in clinical psychology.

Frank Costin of the University of Chicago has accepted an appointment as assistant professor of psychology at the University of Illinois, Galesburg.

Arthur H. Brayfield, formerly dean of student personnel at Long Beach City College, has been appointed assistant professor of education at the Uni-

versity of California, Berkeley. He will direct the recently expanded training program for student personnel workers.

Kathern McKinnon, formerly of Michigan State College, has accepted a position with the Child Guidance Clinic in Great Neck, Long Island.

Elizabeth Fehrer, formerly of Michigan State College, has accepted an assistant professorship at Brooklyn College.

Leonard Mead, on leave from Tufts College to serve with the Human Engineering Branch of the Special Devices Center, Office of Naval Research, has returned to Tufts as associate professor of psychology and research coordinator for the university.

Morris Rubin, formerly vocational advisor at the University of Pennsylvania Veterans Guidance Center, has been appointed executive director of the Philadelphia B'nai B'rith Vocational Counseling Service.

John W. Whitfield, senior member of the Medical Research Council Applied Psychology Research Unit at Cambridge University and member of the editorial board of the new *Quarterly Journal of Experimental Psychology*, is visiting lecturer in the department of psychology at Wesleyan University for 1948–49. He is replacing **Robert H. Knapp** who has a year's leave of absence to make a field study of the undergraduate colleges and departments which have been notably successful in turning out scientists.

Saul B. Sells, formerly of the A. B. Frank Co., Dallas, has joined the staff of the Department of Psychology of the Air University, School of Aviation Medicine, Randolph Field, Texas.

Richard S. Lazarus of the University of Pittsburgh has been appointed assistant professor of clinical psychology at the Johns Hopkins University.

Robert Tyson of Hunter College has been appointed to the advisory staff of *The Journal of Education*.

Ivan N. Mensh, formerly U. S. Public Health Service fellow in clinical psychology at Northwestern University, has joined the department of neuropsychiatry in the School of Medicine at Washington University, St. Louis.

Rutherford B. Porter, who has been director of counseling and guidance at Northern Michigan College of Education, Marquette, is now director of the Special Education Clinics and head of the Division of Special Education at Indiana State Teachers College, Terre Haute.

Leo Charles Ward, Jr. has accepted an appointment as instructor in psychology at the Georgia Institute of Technology beginning in September.

Elmer D. West, formerly director of admissions, associate professor of psychology, and director of the summer session of the University of New Hampshire, and **Geraldine Spaulding,** formerly editor, National Teachers Examination Project, Educational Testing Service, have joined the staff of the American Institute for Research, Pittsburgh.

David G. Ryans has accepted an appointment as professor of educational psychology in the School of Education, University of California at Los Angeles. He will continue to serve as the liaison representative of Educational Testing Service in connection with their Teacher Examination Program, and as director of the Teacher Personality Study.

The Division of Education and Applied Psychology of Purdue University announces the following appointments to its staff: as associate professor, Arden Frandsen, formerly of Utah State Agricultural College; as assistant professors, Kenneth S. Davenport, formerly of Michigan State College; Homer G. Wood, formerly of New Haven Junior College; Ernest J. McCormick and Richard R. Waite; and as instructor, Henry Davis Shanklin, III.

Harold G. McCurdy, formerly of Meredith College, has become an associate professor at the University of North Carolina. The department of psychology at the University of North Carolina for 1948–49 now includes professors John F. Dashiell, Harry W. Crane, English Bagby, Dorothy Adkins and James W. Layman; associate professors William J. Daniel, Irvin S. Wolf, and H. G. McCurdy; and instructors Norman Kaplan, Albert Branca, Howard Page, Marjorie Meyers, Olga Gernova, Winifred

Meldrim, Mary Alice Danziger, Edith Chen, Marie Louise Peachee, and Donald Driggs.

The Division of Psychologists in Public Service announces that five people, two officers and three others, were present for the opening phases of its business meeting, held at Boston on September 9, 1948.

The Centennial meeting of the AAAS was held in Washington, D. C. from September 13 to 17. Attendance of psychologists was small, since their own annual meeting had ended the week before. Several psychologists participated in the program. On Tuesday morning, Lyle Lanier presided at a session entitled "Human Individuality"; L. L. Thurstone of the University of Chicago spoke on "The Primary Mental Abilities," and Muzafer Sherif of Yale University was one of the discussants. On Wednesday morning, on a program entitled "Educability," Arnold Gesell of Yale University spoke on "The Growth Potentials of the Human Infant." The program consisted entirely of invited papers which were designed to summarize current developments in the principal fields of science.

Howard Meyerhoff, professor of geology at Smith College, was elected administrative secretary of AAAS beginning January 1949.

Barney Korchin's address is 2224 Locust Street, Philadelphia 3, Pennsylvania, instead of 1201 Greeby Street, as incorrectly listed in the 1948 APA Directory. In correcting your copy, note that Sheldon Korchin's address should remain as 1201 Greeby Street.

The mental health program authorized by Congress in 1946 received an appropriation of $9,028,000 for the fiscal year 1949. This sum is to be used for financing research on mental illness, development of local mental health facilities, and training of mental health personnel.

The following institutions received grants to cover improvements in the training of clinical psychologists and stipends for selected graduate students:

University of California
Columbia University, Teachers College
Duke University
University of Illinois
State University of Iowa
University of Michigan
University of Minnesota
New York State Psychological Intern Training Program
Northwestern University
Ohio State University
University of Pennsylvania (training stipends only)
Princeton University
University of Rochester
Stanford University
Western Reserve University
Worcester State Hospital
Yale University

In addition, training grants but no stipend grants were made to:

University of Cincinnati
University of Colorado
Harvard University
University of Kentucky
The Menninger Foundation
University of North Carolina
Tulane University
University of Washington (Seattle)

The TAT Newsletter is an informal mimeographed quarterly of interest to users of the TAT. Volume I is still available and Volume II can be subscribed to for the same price, $1.00 per year. Address the editor, Dr. Robert R. Holt, Menninger Foundation, Topeka, Kansas.

Science Research Associates, in conjunction with the department of psychology of the University of Chicago, has established an internship program for students in the department to work half-time on research in various areas of psychology. These appointments will offer opportunities for gathering data for doctoral dissertations and publications of Science Research Associates. One intern, Sarah Counts, has been appointed for 1948–49.

Research Director, Army's Attitude Research Section in Germany. To conduct opinion-attitude studies among military personnel. Knowledge of IBM equipment and of attitude surveys essential. PhD desirable. Salary $9290.25 (Civil Service Grade, P–6, plus living and quarters). Write to Jack Elinson, Army Air Force Information and Education Division, Pentagon, Washington 25, D. C.

Psychologists, State of Minnesota. As vacancies for psychologists occur from time to time, they are filled from the lists of those who have passed the Civil Service examination. Psychologists interested in being notified the next time the psychological examination is given are invited to apply to the State of Minnesota Civil Service Department, 122 State Office Building, St. Paul, Minnesota.

Senior Clinical Psychologists, State of California. Entrance salary $376 a month, with annual increases for satisfactory service to a maximum of $485 after three years. Positions under Civil Service, but temporary appointment may be made pending qualification by examination. Applicants must have education equivalent to the Master's degree with a major in psychology and two years of appropriate experience; the PhD can be substituted for one year of experience. California residence not required. Apply to the State Personnel Board in Sacramento, San Francisco, or Los Angeles, or at any California Department of Employment office. Or write to Dr. Burton M. Castner, Chief, Division of Diagnosis and Classification, California Youth Authority, 401 State Office Building #1, Sacramento, California.

Specialist Commissions in the U. S. Marine Corps Reserve (Inactive) are available to limited numbers of psychologists qualified in personnel research. For applications address the Commandant of the Marine Corps, Washington 25, D. C.

Research psychologists in different branches of the U. S. Government are needed in various research laboratories. These range in level from entering positions to directors of research laboratories. The entering positions require the equivalent of a bachelor's degree with a psychology major and the work is at a junior apprentice level serving as laboratory or research assistant to a senior psychologist. The entering salary for these jobs is P–1, or $2970.80. At the other end of the scale is the Psychologist at the P–8 level at $10,330 a year, who serves as head of a large staff of psychologists and their assistants.

At the present time, all positions are filled from registers of eligibles resulting from competitive examinations conducted by the U. S. Civil Service Commission. Jobs at the entering (P–1) level are filled from the examinations for Junior Professional Assistant. The U. S. Civil Service Commission announced, on September 14, 1948, an examination for the grades P–2 and P–3 ($3,727 and $4,479). There are two optional subjects: (1) experimental and physiological psychology; and (2) personnel measurement and evaluation. Applications to take the examinations for these grades must be made not later than November 9, 1948. Persons interested may secure full information and application forms at most first- and second-class post offices, or from the U. S. Civil Service Commission, Washington 25, D. C.

Applications are still being accepted by the Civil Service Commission for higher-grade research psychologist positions under Examining Circular EC–9, issued on January 14, 1947. The salaries range from $5,232 to $10,330 a year. No written test is required; candidates will be rated on their experience and training. To qualify for the positions at the highest level, candidates must have earned recognition in the field of psychological research.

Clinical Psychologists, Illinois. Two vacancies for the grade Psychologist I, salary $190 per month with a deduction of $32 for full maintenance. MA or its equivalent desired, man or woman. Apply to Dr. William W. Fox, Superintendent, Lincoln State School & Colony, Lincoln, Illinois.

School Psychologist, Wisconsin. Dr. Marjorie L. Page, Board of Education, Whitefish Bay Public Schools, 1200 East Fairmount Avenue, Milwaukee 11, Wisconsin, has listed a position for a school psychologist, preferably a woman under forty. The salary schedule for teachers there is based on education and experience. The range for MA plus 15 credits, for example, is from $2200 to $4100.

Clinical Psychologist, Virginia, wanted for teamwork with psychiatrist and social worker in a mental hygiene clinic under the Federal-State-Local set-up. PhD or equivalent. Beginning salary up to $4,848. Apply to Director, Roanoke Guidance Center, 1912 Memorial Avenue S. W., Roanoke 15, Virginia.

Clinical Psychologist, Texas, wanted for the Texas State Department of Health, Division of Mental Health, Austin 2, Texas. The duties are those of assistant director of the Division of Mental Health—to plan and administer clinical psychology for the state mental health program. PhD preferred, or certification by ABEPP; three years of experience as a minimum. Salary, $4500–$5652. Apply to the Director, Dr. Elizabeth Gentry.

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